it is to reduce the organic matter by putrefaction to soluble compounds. Further, in the coarsely porous and intermittently aërated contact beds, in which the soluble material from the septic tank is allowed to stand, are afforded the best conditions for the action of the aërobic or nitrifying bacteria. Experience with this method shows that even the solid material from the raw sewage, which in the septic tank is differentiated as a scum at the surface and a deposit at the bottom, is also slowly acted upon by the bacteria so that the quantity in the tank is not appreciably changed after the colonies have become firmly established. In the contact beds also the aërobic bacteria establish their colonies in a few weeks and appear in the form of slime, which adheres to the surface of the cinder or other porous material composing the bed. Before passing from the septic tank to the contact beds the effluent is aërated by allowing it to flow in thin films over weirs. "Perhaps never in the history of engineering," said Mr. Johnson, "has a new process, as revolutionary as this one, established itself so quickly with the highest authorities as has this new and simple method of sewage disposal."

In discussing the paper Mr. H. L. Russell likened the earlier attempts to accomplish the bacteriological purification of sewage in a single process to an attempt to raise the subtropical rice and the temperate to subpolar barley in the same field. Mr. F. E. Turneaure also discussed the paper, emphasizing both the cheapness and the efficiency of the new method. The paper and the discussion aroused much local interest, due partly to the fact that the city of Madison against the advice and the urgent protest of members of the Science Club, recently installed an expensive chemical plant for disposal of its sewage, but has now been compelled to abandon it as a complete failure and has elected Mr. F. E. Turneaure to be City Engineer.

Mr. Johnson's paper will probably be published as a Bulletin of the University of Wisconsin.

Officers of the Club for the ensuing year were elected as follows: Mr. E. A. Birge, President; Mr. C. S. Slichter, Vice-President; Mr. E. R. Maurer, Secretary and Treasurer.

WM. H. HOBBS.

DISCUSSION AND CORRESPONDENCE.

REPLY TO PROFESSOR KINGSLEY'S CRITICISM.

ONLY about a week ago my attention was called to the criticism by Professor Kingsley of my little book entitled 'Outline of Comparative Physiology and Morphology' in SCIENCE of April 27th. I delayed answering because I was at that time too much absorbed by many duties connected with the close of the academic year to allow my mind to be distracted by unpleasant matters. I am now at comparative leisure and undertake to show that many at least of his criticisms are unjust.

His points of criticism may be classified under several heads:

1. First and most numerous are general statements which are true but not without exceptions. This was unavoidable in a bare *outline* such as the work professed to be. Our distinctions in science are always sharper than in nature. This is especially true in elementary science. If exact details and all exceptions were given we should certainly fail to give a clear outline to be filled in by subsequent study. Under this head come—(*a*) the failure to make exception of Fungi in giving the broad distinction between animals and plants in the nature of their food. If I had attempted absolute exactness I should have been compelled not only to make exception of greenless plants, but to have discussed the economy of carnivorous plants, and the question whether all plants, even the greenest, do not supplement their mineral food with more or less of organic food. And then what would have become of my *Outline?* The very first necessity in an elementary work is to renounce much, very much that we should like to introduce. (*b*) Under the same head comes the statement that animals by virtue of the nature of their food must have a stomach, without mentioning some exceptions among parasites, as the tapeworm. (*c*) In speaking of the general absence of the middle ear in amphibians I did not make exception of Anura. (*d*) In omitting mention of distinct renal organs in Phyla lower than Mollusca. Surely these objections are hypercritical although in some cases especially (*c*) a foot-note might be added giving exceptions.

2. I said the last group may be regarded as

examples of hypercriticism, but there are other objections which are distinctly so. (*a*) I say, the olfactive nerve is specialized for the perception of odors, he objects that the *terminations* alone are thus specialized. This may be true, but is not the termination also included in my general statement? (*b*) I say a muscle is an arrangement for changing nerve-force into mechanical power. He apparently objects, but why he does not say and I cannot imagine. The statement is certainly true. (*c*) He reproaches me for certain important omissions, *e. g.*, the *mesodermal origin of metameres*. Surely this objection implies an attitude of mind wholly inconsistent with the writing of an outline.

3. A few of his criticisms are pure mistakes or else misunderstandings of my meaning. For example (*a*) he quotes me as saying that 'no voice is known below hexapod insects' and cites the stridulation of spiders and crustaceans. I did not say so. I said '*below this department*,' *i. e.*, *arthropods*. (*b*) I state that homologies are not distinctly traceable beyond the limits of a Phylum. The cases of homologies beyond these limits which he mentions are not certainly examples of homology but of analogy, not evidences of common origin but of adaptive modification. But in any case it must be remembered that I was tracing homology only in the clearest cases and as an argument for Evolution. (*c*) He says I omit all mention of branchiæ in Asterias although I mention them in Echinus. Is he sure that there are any such in Asterias? Perhaps they are among the newest things which he accuses me of neglecting.

4. Some of the objections he makes concern points still in doubt, *e. g.*, the function of the pedicillariæ in echinoids. The function I gave, viz, that of carrying food to the mouth, is still held and is not inconsistent with that which he probably had in mind, viz, the cleansing of the body.

5. Besides these there are, I frankly acknowledge, some real mistakes. It would be strange if there were not. For pointing out these I most heartily thank him. I will profit by his criticisms.

But I fear I weary the reader with personal matters which are of little importance. It is with real pleasure therefore that I hasten on to take up the last point, which is one of great interest in the general field of scientific thought.

6. The worst fault which Professor Kingsley finds in my book is '*the recognition* of a *vital force*.' Now this really amuses me. Surely Professor Kingsley must be ignorant of the early history of discussions on this subject or he would be aware that I was myself among the earliest enforcers of the doctrine of 'the correlation of vital with physical and chemical forces and the conservation of energy in the phenomena of living things.' I even suffered somewhat from the odium theologicum on that account. That my contributions to the discussion were not unimportant see the references given below.* The position I held then is so universally acknowledged now that the history of the discussion has lost something of its interest to the present generation. But some would go farther. In the revulsion against the *old idea of vitality* as an independent supra natural force unrelated to the other forces of nature the scientific mind swung too far in the contrary direction, and it become the fashion for scientific men to ridicule even the use of the term *vital force* as the useless remnant of an old superstition and indicating a wholly unscientific attitude of mind; and thus gradually arose an odium scientificum forbidding the use of the term on pain of being thought unscientific. And yet the same men who repudiate life as a force talk serenely of gravity as a force or chemical affinity as a force, or the force of attraction or of inertia, wholly unconscious of any inconsistency in their position. The fact is, all of these stand on the same footing. They are *none* of them forces in the old sense of independent entities—they are *all* of them forces in the sense of *different forms* of the one universal energy, they are all *derivable from and convertible into* one another. They are all different forms of energy, determined by different condi-

**Am. Jour. Sci.*, Vol. 28, p. 305, Nov., 1859; *Phil. Mag.*, Vol. 19, p. 133 and 243, 1860; *Pop. Sci. Monthly* for Dec., 1873; *Carpenter's Physiology*, 7th ed., p. 7; McGee, Fifty Years of Am. Sci.; *Atlantic Monthly* Sept., 1898.

tions, giving rise, each, to its own characteristic group of phenomena, the subject matter of its own peculiar department of science. For convenience we give them names. Now the group of phenomena characteristic of living things is a more peculiar group than any other lower group, and therefore the determining form of energy *better deserves a distinctive name* than any other and lower form.

But some one will say: "vital force is a metaphysical conception and as such has no place in science." If so, then must we banish also all ideas of force, or power, or cause as metaphysical. The fact is, science cannot get on without metaphysical conceptions. We strive in vain to realize a science such as Comte imagined—a mere succession of phenomena following one another like the trooping shadows of a phantasmagoria *without causative nexus between.* Comte repudiated the idea of atoms and of a hypothetical ether as metaphysical ideas, and yet, who can estimate the service done to science by these ideas?

These views I have maintained for the last 30 years. In spite of the odium scientificum I have continued to use the term *vital force*, not indeed in its old sense but in a true rational sense. But the reaction toward a more rational view is now fairly on. It may again go a little wrong. I cannot sympathize entirely with all the recent views on this subject. Some of them seem to smack a little of the old supra-naturalism, but it will come right in the end. Meanwhile, I would commend to the attention of all who, like Professor Kingsley, are afflicted with a dread of *vital force*, an article in the *Monist* for July, 1899, entitled 'Biology and Metaphysics,' by that acute thinker and lucid writer, Professor C. Lloyd Morgan, as being altogether just. Professor Morgan is admitted to be an exact and painstaking biologist; but he is also what is far better and rarer, a profound and philosophic thinker.

Perhaps I have already said too much. All I can ask is that those interested, unbiased by the fault-finding criticism, will examine for themselves in a fair and sympathetic spirit. I do not fear the result.

JOSEPH LE CONTE.

BERKELEY, CAL., May 24, 1900.

GLACIAL EROSION IN THE WHITE MOUNTAIN NOTCHES.

TO THE EDITOR OF SCIENCE: In *Appalachia* for March, Professor W. M. Davis discusses the glacial erosion of certain over-deepened valleys in the Alps and the relation that is borne to them by the hanging valleys of their tributaries. He suggests that "the head of the Saco valley in the White mountains below Crawford notch deserves examination to see how far its smooth sides and U-shaped cross-section may be explained as the results of glacial scouring by an ice stream that hurried through the deep opening in the White mountain mass." The present note may throw some light on this question.

It is in the first place remarkable that, although there are valleys of east and west trend in northern New Hampshire, all the deeper notches and passes practicable for roads through the main mountain group extend from north to south, as would be natural if the notches had been deepened by ice streams moving in the general direction of the glacial striæ in New England. Moreover, Carter notch as seen from a distance, the Crawford notch as seen from Mt. Willard, and Franconia notch, all present essentially U-shaped cross-sections, their troughs being bordered by continuous cliffs rather than by projecting spurs, thus suggesting erosion in a roughly horizontal direction along the sides of a glacial channel, rather than down-hill erosion by streams on the side slopes. In the second place, if one climbs Carter dome from the notch, the path is so steep for the first eighth of a mile that one must cling to the trees to ascend it; but then there suddenly comes a gentler slope. As a boy I climbed the western wall of the White mountain or Crawford notch by way of the bed of Brook Kedron, south of the Willey House, and found it so steep as to be almost impracticable; but here again a point was reached from which the stream was seen coming leisurely over the plateau south of Mt. Willey before its plunge down to the Saco on the floor of the main valley. Standing on Mt. Willard, one looks east across the notch trough to where the Silver and Crystal cascades slip and leap down over the shining ledges, now and then disappearing in narrow clefts that